most part, at considerable depths, as in the depressions of 70 to 90 fathoms found in Loch Fyne.

I notice that this letter has now been reported in the *Fish Trades Gazette*, and, fearing lest the statement should go further unchallenged, I should like to suggest that the experiences of Brady, Herdman, and Scott cannot be set aside lightly. I in no way doubt the statement that copepods are abundant on the mud at 70 fathoms, but from the evidence given it is surely unnecessary to conclude that such depths are the natural feeding-ground of the herring. In Loch Fyne, after continued calms, I have seen congregations of *Calanus finmarchicus* floating in belts of dull opaque yellow, or left by the tide in such masses as made it possible to scoop them up in one's hand. It is far from probable, I fancy, that Mr. Turbyne's deep-water forms were more abundant, and yet the question is not merely one of the vertical range of copepoda. That herrings descend to a considerable depth seems true. That their eggs will hatch at as great a depth of 95 fathoms, the late Mr. George Brook and I proved in Loch Fyne. If at certain seasons they tend to congregate in deep areas, it is fortunate that, as Mr. Turbyne shows, a favourite food is to be had; yet when the copepods were as I describe, I saw the herrings being caught near the surface—I spent many nights off Skippness Point and in Kilbrennan Sound—and the fishermen told me that beneath such floating belts of copepods, closely packed shoals are usually to be met with. The fishes taken were gorged with *Calanus* and a few other species, such as *Temora longicornis* and *Dias longiremis*. The so-called disease of gut-pock was then prevalent. This I attributed, rightly or wrongly, to excess of oil brought into the intestinal tract by the copepods. The oil extruded plentifully from the anal openings. This, then, is evidence of herrings feeding freely on a plethora of copepods near or on the surface; evidence of a similar kind to that of Mr. Turbyne's, yet with an opposite deduction.

W. L. Calderwood.

Napier-road, Edinburgh, November 11.

MacCullagh's Theory of Double Refraction.

It is quite clear that Mr. Larmor has misapprehended the purport and object of the criticisms in my letter to Nature of October 3.

I have not attempted to discuss the question whether or not "a gyrostatic ether may be constructed which will function (*sic*) according to MacCullagh's optical scheme." What I have said, is that the theory advocated by Mr. Larmor violates the principle of angular momentum, which is a totally different thing. I have also carefully avoided making use of any such phrases as "ordinary elastic matter," "elastic solid matter," and the like; for the employment of such expressions with reference to a quasi-fluid medium of extreme rarity, such as the ether is supposed to be, is altogether misleading, and is responsible for a good deal of misconception upon the subject.

Ever since the undulatory theory was accepted by the scientific world, the object of mathematicians has been to endeavour to form a conception of a medium whose properties are such, that optical phenomena may be deduced therefrom by dynamical principles and methods; and in order that any attempts should be successful, it is necessary for the medium to possess inertia, and also to be capable of resisting deformation. The latter condition requires that the forces on any right solid element, which are due to the action of contiguous parts of the medium, should consist of nine stresses. Now, whatever physical properties we may ascribe to the ether, or whatever the mathematical form of the equations may be which connect the nine stresses with the quantities upon which deformation depends, the principle of angular momentum requires that three relations should exist between the six conjugate stresses, except in the two special cases I have referred to in my former letter; that is to say, unless the medium is a gyrostatic one, or unless it is under the action of some system of forces (mutual or otherwise) whose action upon an element consists of a couple as well as a force.

Mr. Larmor appears to think that a satisfactory solution can be obtained by postulating an energy function, and deducing the equations of motion and the boundary conditions by the principle of least action; but this view is fallacious. For if some particular form of the energy function were assumed, and were found to lead to results which violate the principle of angular momentum, the theory would be dynamically unsound, and the results would represent some impossible form of motion. To write down certain mathematical expressions, and to perform certain mathematical operations, do not constitute a satisfactory theory, unless it can be shown that all the results furnish a consistent scheme in which none of the fundamental principles of dynamics are violated.

Mr. Larmor also suggests that possibly the energy function of a gyrostatic medium might be modified[1] in such a manner that MacCullagh's equations might be deduced therefrom by means of the principle of least action, and that it would be interesting and instructive to establish this in detail. But why has this not been done? If a mathematical investigation of this kind would be unsuitable for Nature, the same objection would certainly not apply to the *Philosophical Transactions*.

I shall defer entering fully into the subject of magnetic action on the present occasion; but in the paragraph marked (2), Mr. Larmor has altogether failed to deal with the objection which I advanced, viz. that his theory is open to exactly the same defect as my own, inasmuch as it makes the tangential component of the E.M.F. discontinuous at an interface. What, according to Mr. Larmor's view, is the mathematical expression for the E.M.F. in a magnetised medium? What is the physical interpretation of the boundary condition referred to in my second letter? Does he propose to modify Maxwell's equations connecting the E.M.F. with the electric displacement? If he does, he ought to have stated the fact, explained the nature of the modifications he proposes, and his reasons for adopting them.

In conclusion, I fail to see that Mr. Larmor has made any adequate reply to my criticisms. To do this he must show (1) that his resuscitation of MacCullagh's theory does not violate the principle of angular momentum; (2) that his magnetic theory makes the tangential component of the E.M.F. continuous at an interface.

A. B. Basset.

Fledborough Hall, Holyport, Berks, November 12.

The Nomenclature of Colours.

The letter of Mr. Herbert Spencer in your issue of August 29, suggesting the use of a nomenclature to be used in connection with a scheme of colour standards, on a plan similar to that used by sailors in boxing the compass, is interesting, both because of its obvious appropriateness and because, as Mr. Spencer suggests may be the case, it has occurred to others. In a paper read before the American Association in 1890, I suggested the same idea, but at the same time stated that this would probably give more hues between the six standards than would be convenient in view of the uncertainty that will almost surely result in the mind of the common observer as to just which term should be applied in any one particular case. In view of this, I thought it wise to make the number of hues between the standards smaller than the compass chart would give. Those who have applied the standard colours to educational work have found this desirable.

The Milton Bradley Company, in their educational series of colour papers, have introduced only two hues between the standards. This makes the series R OR RO O YO OY Y GY YG G, &c. Mr. Louis Prang, of Boston, some two years since published a small pamphlet, proposing to make the series for common use R RRO ORO O OYO YO YYO Y, &c. On the whole, I think it wise to adopt the smaller number for ordinary use, and interpolate others for more delicate discriminations. A plan which seems to me a very practical one is the following, in which the hues represented by the symbols in capitals are for ordinary use, and those in small type for more careful discrimination: R orr ror OR oor rro RO oro roo O yoo oyo YO yyo ooy OY yoy oyy Y, &c. Translated into words, this would be RED, orange-red red, red orange-red, ORANGE RED, orange orange-red, red red-orange, RED ORANGE, orange red-orange, ORANGE, &c. Whatever method is used, however, does not in any way affect the value of the scheme of definitely determined standards of colour as proposed in my paper.

J. H. Pillsbury.

Stoneham, Mass., U.S.A., November 7.

The *Scientific American*, of New York City, in its issue of October 19, 1895, quotes from a letter to Nature (vol. lii. p. 413), written by Mr. Herbert Spencer, suggesting that a

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[1] The theory of the mixed transformation of Lagrange's equations, which leads to the energy function alluded to, was first given by myself in 1887. (See *Proc. Camb. Phil. Soc.*, vol. vi. p. 117, and "Hydrodynamics," vol. i. p. 173.)

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system of colour nomenclature might be devised by indicating colours in a manner analogous to the accepted nomenclature of the points of the compass.

In this connection the enclosed circular, published by me during the summer of 1893, to explain briefly my system of colour standards, may be of interest to your readers. You will see that the system of colour nomemclature here described corresponds almost precisely to the idea in Mr. Spencer's mind.

October 24. LOUIS PRANG.

[WE have received, with Mr. Prang's letter, the circular descriptive of his system, which is essentially the same as that suggested by Mr. Spencer. As Mr. Spencer pointed out in his letter to us, the idea is a very obvious one, and had probably occurred to others; but this does not, of course, diminish its value. For ourselves, we are glad to see that the idea has been put into practice, and that, out of a chaos of colour-names, an intelligent system of nomenclature has been evolved. A series of standard coloured papers, harmoniously bound together, is published by the Prang Educational Company, Boston, for the use of teachers, designers, artists, and others.—ED. NATURE.]

RECENT IMPROVEMENTS IN LIGHTHOUSE ILLUMINATION.

PRIOR to the year 1822, the optical apparatus used in the best equipped lighthouses was silver-plated parabolic reflectors, having apertures, twenty-one inches for fixed, and twenty-five inches for revolving lights, their focal distance being four inches. These instruments, with a burner one inch diameter, and an initial power of eleven candles, condensed the light into a beam of 1304 and 2360 candles respectively. The power of a first order reflector light of sixteen reflectors, ranged on a frame having four faces, was equal to 9340 candles. These were the instruments (Fig. 1) which A. Fresnel, the celebrated physicist, replaced by optical apparatus of glass lenses and prisms acting by refraction and reflection in combination with a central lamp and multiple-wick burner of considerable power. In doing so he devised four optical agents: the annular lens, totally reflecting prisms, the cylindric refractor for small apparatus, and straight vertical reflecting prisms. These instruments he combined in different ways to form fixed and revolving apparatus suitable for lighthouse purposes. The annular lens subtended at the focus 45° vertically and horizontally, the focal distance being 920 m.m. (36·22 inches). Some of Fresnel's optical instruments in the hands of Mr. Alan Stevenson, underwent very notable alteration, extension, and improvement. The first order lens was extended to subtend 57° vertically, and reflecting prismatic rings were substituted below the lenses for silvered mirrors. The central portion of the first order fixed light apparatus (Fig. 2) was converted from a polygon formed of narrow lenses to a cylindrical drum, which drum was divided into sections having helical joints, and above and below the central belt reflecting prisms were introduced.

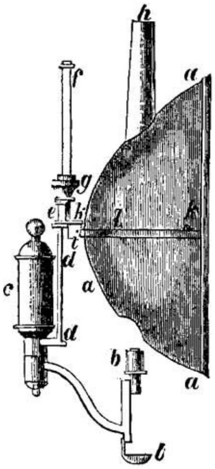

FIG. 1.

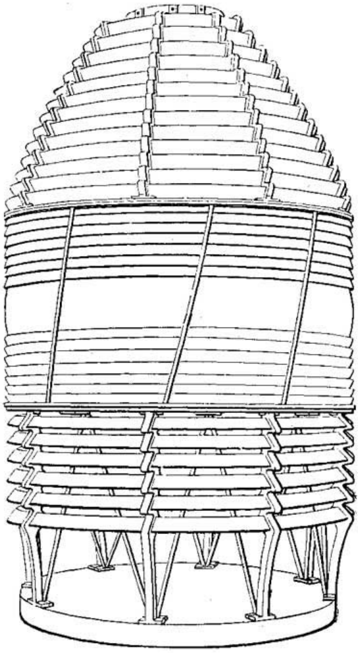

FIG. 2.

The next improvements were those of 1849–1850, by Mr. Thomas Stevenson, the main feature of which was that, for revolving lights, totally reflecting prisms generated round a horizontal axis were introduced above and below the lenses, lenticular action being thus extended throughout the whole height of the instrument (Fig. 3).

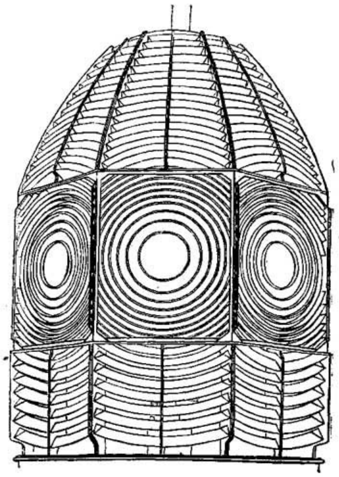

FIG. 3.

Then followed the dioptric spherical mirror, the dioptric holophote, the azimuthal condensing system, and its application to fixed, flashing, intermittent, and group flashing lights; the bivalve apparatus of 1859, which is really two holophotes ten feet in diameter, increased in power by a dioptric mirror, and numerous combinations of these and previously devised instruments.

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